and that the necessarily prolonged anæsthesia is too dangerous to be generally employed.

"Litholapaxy," applied to a very limited class of cases, is a very good operation, in the hands of skilled and experienced lithotritists, but too much of an undertaking for beginners or for those who are not constantly occupied with surgery.

Respecting lithotrity in children, only a few words will be said.

Lithotrity was successfully applied in 1827 by Civiale to an ill-developed child 7 years of age suffering from a large vesical stone. In 1834 Ségalas reported a case of stone treated by lithotrity in six sittings in a child aged 33 months, the stone measuring "11 lines." He subsequently recorded four additional cases in children aged respectively: 40 months, stone 10 lines, four sittings; 45 months, stone 13 lines, ten sittings; 4½ years, stone 1 inch, six sittings; 5 years, stone 15 lines, twelve sittings in two months; and no deaths. In 1838, Nathan R. Smith performed lithotripsy successfully upon four boys, of whom one was under 2 years of age, another 3 years, and the other two 7 years. In 1839 John Randolph performed this same operation upon two boys, one aged 4 years and the other 11 years, both cases being successful. In 1836, Leroy also wrote of lithotrity in young children, and Guersant, who operated upon twenty-one children, lost six. Mercier (1862), Beyran (1863) Fournier (1874), Gonzalez (1883), and others, all spoke their word concerning lithotrity in children, and the final conclusion of those whose testimony carries the most weight was that it should be reserved for cases of small stones requiring only one short sitting. Sir Henry Thompson, whose number of lithotriptic operations exceeds that of any other living surgeon, records, up to the year 1884, only three lithotrities upon children.

Dr. Keegan's success with lithotrity and "litholapaxy" in children is likely to lead many young surgeons to the wrong use of these operations, and many will be the ill consequences of such a violent procedure as "litholapaxy." In some of the children subjected to "litholapaxy" by Dr. Keegan the detritus weighed only a few grains and could have been easily expelled spontaneously after simple lithotrity, and he counts these cases among the triumphs of "litholapaxy;" and in other cases the stones exceeded 200 grs. in weight. In the first instance, simple lithotrity was clearly indicated, while in the second, both lithotrity and "litholapaxy" were clearly contraindicated. Of Dr. Keegan's first seventeen cases of "litholapaxy," in children aged from 20 months to 12 years, only one patient died. This speaks well for the tolerance of the patients' bladders and for the skill of the operator, but even if the percentage of mortality should, in future, be no greater in other cases of children between the ages of 20 months and 12 years, it would not render the operation justifiable in children of 5 or under 5 years of age.

Lithotrity would long ago have been largely performed in such children if it had been found generally safe. In cases where the stone is friable, small, weighing a few grains, lithotrity is not only justifiable, but the proper operation, provided the urethra easily admit a suitable lithotrite; but to combine aspiration with such an operation is, to say the least, unnecessary.

Lithotomy is assuredly the safer operation in such children when the stone exceeds ½ inch in diameter, but when it is too large to be extracted, lithoclastic cystotomy is essential to success in the majority of cases.

Sir Henry Thompson gives a table in which it appears that of 473 lithotomies performed upon children of from 1 to 5 years of age, thirty-three died, or one in fourteen and one-third cases; and of 377 operations upon children of from 6 to 11 years of age, sixteen died, or one in twenty-three and one-half cases. Lithotrity in children has not yet given such results on the same scale.

The general conclusion drawn from this study is that a proper choice of treatment of urinary concretions can only be made after careful consideration of each method and each case, there being no generalizable method.

YELLOW FEVER.

Read before the State Medical Society of Arkansas, Pine Bluff, Meeting May 28, 1889.

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With your kind permission I shall read a brief paper on yellow fever, a theme at once interesting to all students of medicine, and presumably of marked interest to my *confrères* in the "Sunny South."

In what follows I shall refer to my own experience of this dread disease at Panama on the Pacific, Colon on the Atlantic, both ports of the Isthmus of Panama. My studies and observations on the west coast of Mexico, where in 1885 I traced its epidemics of 1883 and 1884. My experience in the hospitals of Cuba, and finally my visit to Florida, in the fall of 1887, when I deliberately forecast the epidemic that swept Jacksonville in 1888. My letter of warning to the people of Florida was published in the *Times-Union*, of Jacksonville, November 30, 1887, and was recalled *when the disease was upon them.*

Now, to return to the Isthmus of Panama, where I lived and practiced from 1880 to 1885. I was back there twice in 1886 and twice in 1888, thus, to use an expressive phrase, bringing my knowledge down to date.

The yellow fever of the Isthmus of Panama I

describe thus: It is an acute infectious disease, a specific fever, ordinarily not contagious, but under certain atmospheric conditions, not yet fully explained, the disease undoubtedly develops contagious properties and epidemics result.

Yellow fever is ushered in in a variety of ways. It may be preceded by languor or malaise. The invasion may be abrupt. Generally characterized by a chill, often very severe, lasting one, two or three hours; the duration of the chill having a marked significance, severe chills marking nearly all fatal cases. Again, the disease may be ushered in by sudden nausea and faintness, without any warning, as in my own case during the Isthmian epidemic of 1880. Headache is always met with. I know of no exception to this statement. Frontal headache, a flushed face and gastric irritability in newcomers within the yellow fever zone is always very suspicious, a fact specially referred to in Dr. Belot's admirable book, *La Fièvre Jaune à la Havane*. Generally the headache is frontal; it may be bi-parietal and occasionally occipito-frontal, but, to repeat, marked headache always. In dealing with specific yellow fever of the Isthmus of Panama—of which, if respectability depends on its antiquity, is the oldest, most respectable and fatal variety known—a history of constipation obtains in nearly all cases. I can recall but a single case in my practice where the disease had been preceded by malarial diarrhœa. No condition of health gives immunity. It aims at all, be they healthy or unhealthy. It has a specific rôle. From early youth to advanced age it pursues its death-dealing mission. It is true that the mortality among children is less than at puberty and beyond. Pains in the legs and sacral region, the latter often intense and agonizing. I shall never forget my own experience. It seemed as if a legion of fiends were trying to dig out—if I may use the expression—my sacrum with red-hot pincers. The pain is excruciating and indescribable. In the majority of patients, the face was red, just like the face in scarlet fever—the boiled lobster color. The eyes at first were clear, providing there had been no antecedent hepatic disease; later they became suffused, injected. The skin was hot and dry. In many cases a peculiar biting heat was felt (like the *calor mordax* of pneumonia). It produced a strange sensation, resembling a current of electricity playing over the extended palm. Pulse hard and slow, varying from 65 to 80. Temperature, first stage 100° to 103°, where the cases proved fatal in the first stage, rising to 104°, 106° and 107°, the latter being the highest temperature noted by me in my practice. To fall slightly just before death. In the second stage or "period of calm," as it is termed, it fell, a remission only. At the beginning of the third stage or the stage of "secondary fever" it rises again. Respiration, as one would expect during the "hot stages," is hurried. At times a peculiar moaning respiration of indescribable sadness. It fills the room and the vicinity. The respirations varied from 30 to 40 per minute, and at the close of the third stage 50 to 60, becoming less with the fall of the temperature just before death. Great thirst, nothing appeases it. Restlessness, no position giving any ease. Urine, at invasion, normal but high-colored. In the majority of cases on the Isthmus of Panama they died during the first stage. Such was the blood-destroying intensity of the disease, when all, or nearly all of the symptoms detailed and to be detailed, appeared. They do not appear in any stated order.

Within twenty-four hours of invasion, all the symptoms are intensified. Sacral pain and headache increasing. Gastric disturbances and epigastric tenderness developing early in many cases, the slightest pressure over the stomach causing intense pain and eliciting sharp cries. In cases where the brain symptoms were very marked, in some where patients were unconscious, the slightest pressure produced a contortion of the face and body. If deep-seated pressure was made they writhed upon their beds, but the instant that it was removed they became quiet again. Next, nausea and vomiting, at first a clear fluid, well named "white vomit" by Surgeon-General Blair, of British Guinea, South America. Tongue at first slightly coated. I am dealing with complicated cases. In patients who had suffered from intermittents, or bilious remittents, what is termed the characteristic tongue of yellow fever was not found. As stated, it was slightly furred. Later the fur increases from behind forwards, the tips and edges take on a deep red. Gums also become a fiery red, also the mucous membrane of the mouth and throat. The whole mucous tract suffers. Later, in the majority of cases, sore throat is complained of, due to stripping of the mucous membrane. Blood oozes from the denuded tongue and gums, giving an indescribable fetor to the breath; at times it collects on the teeth. In some cases a peculiar and characteristic odor is exhaled from the patient's body. Once recognized, it never will be forgotten. It somewhat resembles *l'odeur du cadavre*, of French authors. The late Dr. Stone, of Louisiana, was the first American writer, I believe, to recognize it. As he states, it is a very bad omen.

When patients die in the first stage, the urine always shows a large amount of albumen. The temperature remains high, 104° to 107°. Delirium, often quiet, marks the latter temperature. In some cases extending over more time—beyond the fourth or fifth day—the albumen does not appear until the close of the second or the beginning of the third stage. Albumen is a *sine qua non*. I know of no yellow fever without it, nor do any of my many friends practicing within the tropics. It never was absent in Isthmus cases. I never have seen or heard of a case of specific yellow

fever without it; never, either in the practice of Dr. L. Girerd, late Surgeon-in-Chief Panama Canal Company, in that of Dr. Didier, of the same service, or in the cases seen by my brother, the late Dr. George W. Nelson, at one time my partner, and later Resident Surgeon at the Canal Hospitals, Huerta Galla, Panama, giving a combined experience of hundreds and hundreds of cases. During an epidemic at Colon in the fall of 1883, it swept the shipping, over 150 cases, nearly all fatal. Again albumen in all cases. Suppression of urine is a late, and generally among the last symptoms. Where it is marked, they seldom recover. The bowels, if freely acted upon by the sulphate of soda, to be referred to, may not furnish any early information, diarrhœal motions produced by the soda being followed by "black vomit motions" in many fatal cases. These motions may precede or follow black vomit. No rule is absolute, or such material, well named, may only be seen at the autopsy. Black vomit follows the constant retchings and the "white vomit" of Blair. Black vomit is happily named, and shows innumerable fine particles or flocculi named black vomit or "coffee-ground vomit," or the *marc de café* of the French writers, whose books on yellow fever are among the latest and very best. Frequently patients, without the slightest warning, commence violent vomiting. It pours forth from mouth and nostrils, often threatening to choke them. I have seen a patient resting quietly on his back after the subsidence of the gnawing sacral pain, when a perfect flood of black vomit has spurted from his mouth and nostrils up into the air, over bedding, mosquito curtains and the nurse. An old and intelligent writer on yellow fever, Dr. Dowell, has been singularly happy in his remark, that it is *per saltem*. So it is.

Here, I must pause and divide my yellow fever cases into two classes, and shall state that such are met on the Isthmus of Panama. One class, I took the liberty of naming "uncomplicated," the other "complicated." By uncomplicated, I mean the disease occurring in new comers. In these, brain symptoms and delirium were common. Such, almost without the classic exception, died, I never knew one to recover. The possession of full health meant rich blood, and a better culture fluid for the germs, that destroy it; the absolute destruction of the blood being but a matter of three or four days. I can best illustrate this by a case in the practice of my valued friend, Dr. L. Girerd, to-day a retired practitioner living in Paris. In the case referred to, on the fourth day of the disease, he failed to get a single red corpuscle in the blood—not one. The heart was driving a fluid through the vessels—one incapable of nourishing the brain and tissues. A fluid wholly devoid of the life sustaining oxygen carriers, the red corpuscles. His crucial microscopic work revealed a fluid, and in it the débris of corpuscles; or, to use the old time word that I have applied to this condition in yellow fever, a necremia, or death of the blood. His patient, a titled foreigner, a magnificent specimen of manhood, who stood 6 feet 4 inches in his stockings, died a few hours later. The "complicated" occurred in those who had been on the Isthmus from six months to sixteen years, and of course were profoundly malarious. I say of course, as no man, woman or child there escapes intense paludal poisoning. Sixteen years had failed to give the so-called acclimation to an American, Captain Dean. Specific yellow fever cut him off. He was my patient. An elderly Italian, M. Georgetti, after thirty-seven years' residence at Panama, died of specific yellow fever. I personally know a French gentleman in Guaymas, Mexico, who has spent over forty years on both coasts of Mexico. He went through epidemic after epidemic unscathed, but in the thirty-sixth year of residence, after passing through the Guaymas epidemic of 1883, he came down with the disease in 1884, when a few cases appeared, as is usual following *all epidemics within the tropics*, and just escaped dying. He in person related his experience to me. Acclimation is only so-called; it is a myth, but quite in keeping with much of our gross ignorance regarding yellow fever. Nothing, absolutely nothing, protects against yellow fever—except having had the disease, a fact well known to all close students of the disease within the tropics.

With this digression as a preparatory statement, I shall next consider the second stage, or "period of calm," as it is termed. There is a marked fall of temperature, but merely a remission, and most deceptive and dangerous it is. I can best illustrate this by actual cases. In two cases, both mine, during the epidemic of 1880; new arrivals, just married; he a Frenchman and Consul for France; she a Portuguese, aged 17. They had passed the first stage. His temperature had run up to 106°, hers to 105°. Then came the deceptive "period of calm." They felt so well that, despite my emphatic orders, they got up and walked about. He was in one room and she in another. In the woman's case, the secondary fever came on that night, together with a copious "vaginal hæmorrhage," practically, the equivalent of black vomit. She died within twelve hours of her walking about her rooms. His temperature again ran up; he died the next day. She, poor girl, was laid out in her wedding finery. They occupy a single grave in the foreign cemetery at Panama. Such, gentlemen, is malignant yellow fever as I know it.

As I have stated, yellow fever may be a disease of a single "access" or paroxysm. When it is so, the patient dies or enters on convalescence—such being the milder cases at Panama. Thus, it resolves itself into a sharp, clearly defined fever of a single paroxysm, or "access," as the

French so expressively term it. As nearly all attacked died, the milder cases were the exceptions. In the great majority the "period of calm" was deceptive, the slightest imprudence on the part of the patient ending in death later. The remission —I have seen the temperature as low as 99°—lasting from twenty-four to thirty-six hours, in cases marked by long chills, but twenty-four hours, to merge into the third stage of the disease, or that of "secondary fever." I have faced three epidemics of small-pox, one at home in Montreal and two at Panama. The severe chills in that disease, initiating the severe and confluent cases, the high primary fever, the second stage, to merge into the high temperature of the secondary fever, consequent blood changes and death. These cases, so familiar to me, have caused much thinking in connection with my studies in yellow fever and its blood changes. In a fatal case of confluent small-pox, without the slightest warning, I have seen a fluid, that to the eyes was indentical with black vomit, spout from the mouth, high in air, over everything, staining the bedding just like black vomit; it was *per saltem*. To our life-currents we must look for information.

In the "third stage" the albumen appears, that is, if absent at close of "period of calm," it is invariably met here. Black vomit, and black vomit motions, suppression of urine, brain symptoms, etc., in cases ending fatally in this stage, all the symptoms crowd each other, and death closes the scene.

In "uncomplicated" cases, or where violent delirium may be met, many painful scenes result. A young Englishman, a picture of health, as attested by his magnificent physique and rosy cheeks, was stricken on landing. He was my patient. The case closed with furious delirium. Four men had to take turns in holding him, until death closed one of the saddest of sights.

A few words regarding the "fever of acclimation" of some writers. This, mark you, is generally preceded by a slight chill, a rapid pulse, a flushed face, suffused eyes, with a trace of albumen in the urine—in a word, it is a very, very mild form of yellow fever—the febrile movement lasting twenty-four to thirty-six hours, the mildest form of an "access." Failing a trace of albumen—it is not a fever of acclimation, that is to a tropical physician—and the other symptoms, no subsequent protection may be expected. In fact, some profound students of the disease within the tropics, consider it but a temporary protection, that in seasons of epidemic, while such are exposed in a lesser degree, still they are liable to contract the severe type.

Such, briefly told, is yellow fever on the Isthmus of Panama. I have seen and attended it in both cities, Colon and Panama. I wish to add, that it and other tropical diseases have caused, at a low estimate, fully 20,000 deaths on the line of the Panama canal. The *New York World*, of May 18, 1889, credits the French Consul at Colon with saying that 15,000 Frenchmen have died. This probably is a mistake. I believe 20,000 all told, will be a generous estimate. The heaviest dying known to me was in November, 1884, durthat epidemic at Colon, in the shipping and on the Isthmus. In an article in *Harper's Weekly* of July 4, 1885, I placed the death-rate for that month at 653 officers and men of the Canal Company. I obtained the figures from an inside source. The Canal Company's statements, as published in *Le Bulletin du Canal Interoceanique*, were as mendacious as they were misleading. DeLesseps' last ditch, that absurd creation of a man in his second childhood, has cost 20,000 lives, over $200,000,000 in gold, has ruined hundreds of thousands of petty investors in France. Up to the hour of the crash, DeLesseps, in person, while knowing the whole truth, unblushingly told his fictions. Since 1884 he has known the whole truth. He is a wicked old man, who should be buried alive under his fictions.

Many of our confrères have fallen on the Isthmus. Some noble fellows are buried there—yellow fever, dysentery and pernicious fever. Yellow fever must be seen and studied in its own habitats. The Isthmus is one of the earliest.

My visit to Tampa, in November, 1887, impressed me in many ways, but what greatly interested me was to hear of cases of *non-albuminuric yellow fever*. These cases of so-called yellow fever, I believe, furnish that class of people who have had yellow fever two and three times. As may be inferred, I have no faith in any yellow fever without the invariable presence of albumen in the urine. I have yet to meet with or read of a well authenticated case of secondary yellow fever. Nor do I know of a single physician who has seen one.

Now I come to the subject of treatment; and here I most emphatically state that yellow fever has no treatment properly so-called. The host of so-called treatments justify my statements. How can a disease, according to the old view, characterized by the symptoms described by me, have one? Four centuries seem to have taught the profession nothing, or next to nothing. All that was known with absolute certainty was that people got yellow fever and died; the world heard of the dying, and that from Cuba it makes periodic invasions of the Sunny South. The treatment of yellow fever is purely symptomatic, my early treatment, up to 1884, was that of the "Old School." May God forgive it for its ignorance and charlatanism! Many authors have made a *rechauffé*, or re-hash, of the experience of others, they never having seen a case themselves. They are responsible for much ignorance, *if not worse*. Having tried all the so-called orthodox treatments, I, previous to the fall of 1884, settled on the following:

On being called to see a patient at the outset, I played a trump card and made quinine a diagnostic agent. We must bear in mind that a few hours in such cases may mean a life saved or lost. The following was the mixture:

℞

Quin. Sulph	ʒj
Acid. Sulph., Dil. B. Phar	ʒij
Soda Sulph.	ʒij
Tinct. Card Co	ʒij
Water, add.	ʒviij

℥ fiat mistura.

Sig.: Take a quarter at once and repeat in two hours.

This mixture, given French fashion, in *potions*, or portions, well diluted with water, made a perfect solution and was readily absorbed. It was my "multicharge gun." It gave me the best results. Hot baths. Pilocarpine in one case, aconite, etc., were in order, to produce free action of the skin. If the cases were purely malarial the quinine and sulphate of soda met all the indications. The sulphate of soda acts like a charm, free, bilious motions following. Every dose contained 15 grains of quinine and ½ ounce of sulphate of soda. If after two doses the temperature remained high, 100° and upwards, with the usual symptoms, yellow fever was the verdict. Valuable time had been saved, the bowels freely acted upon—a most important indication. Later, I added to this treatment the following: A phosphoric acid mixture every hour or two, largely diluted with water; gave it and it only, purposely to bring about an acid condition of the blood. In a few words, to make it wholly uninhabitable to the germs. I adopted this course, only after serious thought, and said to a medical friend, "My next patient with yellow fever gets well or dies on phosphoric acid." I explained it to two friends, Dr. L. Girerd and Dr. Arthur Gore, who saw my cases. Also, to Dr. Bransford, United States Navy, who crossed the Isthmus on his way to Nicaragua. Previous to my adopting this purely acid treatment, following the quinine and soda mixture, my patients kept on dying in a way that was simply appalling. Not that I lost more than my confrères. Our helplessness dazed me. As stated, after mature deliberation, I settled on phosphoric acid, well diluted, for life or death. Three cases so treated, all in succession, got well, an absolutely unheard of thing there. I had friends see them—knowing as I do, what unbelief and professional jealousy will do. My reasoning was sound. The acid did not destroy the oxygen-bearing function of the red corpuscles, while the germs of yellow fever did, and so killed my patients. By rendering the blood acid these germs could not live and reproduce. They were destroyed *in situ*, and the blood ceased to be a culture fluid. Any student of medicine familiar with bacilli and their cultures knows full well, that even faintly acid solutions are fatal to the propagation of bacilli. Such was my reasoning as far back as 1884. I have the notes on those cases. I took full notes on all my cases, as I had been taught to do, while a student at the Montreal General Hospital, 1868–72.

The blood is the habitat of the germ of yellow fever. When my first case in the series of three demanded my attention, alas, I could not procure a reliable phosphoric acid, when I had to fall back on a formula published on page 93 United States Dispensatory, being that proposed by Mr. James T. Shinn, *American Journal of Pharmacy*, October, 1880, thus: "*Liquor Acidi Phosphorici.* A similar preparation under the name of Horsford's Acid Phosphate has a large use in this country. The formula is as follows: *Liquor acidi phosphorici* (without iron): Calcii phosphat., 384 grains; magnesii phosphat., 256 grains; potassii phosphat., 192 grains; *acidi phosphorici* (60 per cent.), 640 minims; aq., q. s. to make a pint." As stated, not being able to secure a reliable phosphoric acid, I was forced to use Horsford's Acid Phosphate. It, as I knew, was a standard preparation of uniform strength and excellence. I strongly object to employing a patent preparation, so to speak. Its contents or make up was known and it was "Hobson's choice." The preparation did all that I anticipated, and I give its formula as found in the United States Dispensatory. I know what I used. It is essentially a strong acid mixture.

To repeat, having given my quinine and sulphate of soda mixture, thus securing free motions from the bowels. The malarial element being eliminated by the non-effect of the quinine, I then treated for yellow fever, thus: To bring about free action of the hot and burning skin was absolutely necessary. As stated at first, I tried hot baths, aconite, etc., and abandoned them, using a simpler and more effective means, in a vapor bath, named in Peru "Dr. Wilson's treatment," being that of an English physician, who used it with great success during an epidemic there in 1854 and later. The patient was placed on a chair—one with a wooden seat—all clothing being removed; he was covered with blankets tucked in closely under the chin. A spirit lamp was lit and placed under the chair, thus giving heat and vapor. To Dr. Wilson's vapor bath, I added a foot bath, all under the blankets, the water as hot as the patient could bear it. Finally I grafted on some Jamaican treatment, giving a pint of hot lemonade or orange-leaf tea. Under this triad a profuse perspiration followed, usually within ten minutes, it fairly ran off them. As soon as it was freely established they felt better. The scarlet hue of the face faded. The hard pulse became softer. If the bath caused any tendency to faintness, that was guarded against by a shorter exposure. With this I had no unpleasant symptoms, but with nitrate of pilocarpine profound pallor and faintness in a well nour-

ished man caused me alarm. I tried it in but a single case, and that was previous to my knowing of Wilson's vapor bath. The necessary exposure being made, ten to fifteen or twenty minutes, the patient stood up, the chair was slipped from below the blankets and he was lifted into bed *en masse* to prevent any escape of heat or moisture. More blankets were put over him. In some cases the perspiring lasted one or two hours, to the marked relief of the patient and the lessening of all the symptoms. After a variable time the skin again became hot and dry, when the same procedure was repeated as often as necessary. Thus two highly-important indications were met at the very outset. First, under the quinine and soda, free motions from the bowels were secured. Remember the marked constipation in these cases, often extending over three or four days, while the man had been eating as usual. Secondly, full and free action of the skin. According to my way of thinking and reasoning, the patient was placed under the most favorable conditions for fighting the disease. Generally large quantities of fœcal matter were voided, and the pores were thoroughly opened. Next, the rest of the treatment was in order. It was of the simplest. A teaspoonful of the acid phosphate in a half-tumbler of water every hour or two, day and night, for the first twenty-four hours. It never caused nausea. I continued it for two or three days, according to temperature and symptoms. The bowels continue to act freely—bilious motions. Later they became very dark under the acid. Previously I had used sinapisms and a lot of things recommended by the books, and those supposed to be experienced in treating the disease. The sinapisms were placed over the stomach to try and check the distressing vomiting, at times they were beneficial; again, useless. Diet in these cases is a matter of very small importance. They are too busy with the disease. I fail to recall a single case where food of any kind was asked for. The highly irritable stomach must be remembered. Iced milk and beef broth in very small quantities at frequent intervals, *if the stomach tolerates them.* Iced lemonade and pure soda-water. Small pieces of ice allowed to dissolve in the mouth. I gave champagne a fair trial and abandoned it. I am satisfied that the purely acid treatment is ample. The simpler the treatment the better. The quinine and sulphate of soda mixture, vapor baths, *à la* Wilson, and the acid meets all requirements. I abandoned the old-time treatment. As I have already informed you, I had three recoveries, one after the other, all in infected premises where the previously attacked had died. These recoveries were in the fall of 1884. Early in the spring of 1885—March—I left for my annual holiday, visiting Nicaragua, when I returned to the Isthmus, to leave it, April 25th, for New York City.

Three swallows do not make a summer, nor do I claim that three successive recoveries are everything, but as nearly all attacked died, I do earnestly claim that three successive cases getting well furnish food for thought. Personally I am satisfied that by persistently acidulating the life-currents they ceased to be blood-heat culture fluids for the germs of yellow fever. I say germs. The following facts will I believe strengthen my claim that three successive recoveries were absolutely unheard of at Panama. A few words regarding the dying from yellow fever thereaway. I can recall twenty-seven admissions to the yellow fever ward of the Canal Hospitals, Panama, with but a single recovery. My brother, the late Dr. George W. Nelson, then Resident Surgeon, furnished me with the figures. Of 42 cases sent to the Charity Hospital, Panama, during the epidemic of 1880, when I had the disease, not a single recovery. As a concluding statement, I could amplify them to any extent—the Dingler Expedition and its experience will be ample. Mr. and Mrs. Dingler, accompanied by Mr. and Miss Dingler and a party of Canal Engineers, all told, a party of thirty-three, arrived at Colon in October, 1883, Mr. Dingler being the new Director-General of the Canal Works. Within six weeks of landing Count de Cuerno and Mr. Zimmerman were dead—specific yellow fever. Within fifteen months of the landing of that party of thirty-three, fourteen had had yellow fever and but one recovered (Mr. Dingler losing his wife, son and daughter), he was a patient of mine, a Canal Officer, and had been on the Isthmus previously. His regular life no doubt was the factor that saved him. Contrast three successive recoveries with the above—my cases were specific yellow fever.

As previously intimated, yellow fever spares none. While it is quite true that total abstainers have been swept away by it, it is equally true that even in the severest cases, they have recovered, where the moderate drinker was lost from the start. Time and again my own experience has confirmed this. The regular life, particularly within the tropics, is its own reward. In Ziemssen's Encyclopædia, Vol. II, in the article on yellow fever much valuable information will be found on this subject, the value of total abstinence—"Panama in 1855," Harper Bros., New York. Dr. Otis' work, "The Handbook of the Panama Railway," 1860, Harper Bros. Dr. L. Girerd's work on Panama, published in 1883, in French, in Paris, all contain much information regarding that land of pestilence and death, as well as "Five Years in Panama," 1889, Belford, Clark & Co., New York.

In reference to the inestimable benefits of total abstinence within the tropics, it simply confirms the opinion of a valued friend at Panama. The Consul-General of the United States, who, when asked, "How do you live in the tropics?" wittily

replied, "It all depends on the *liver*." So it does. An alcoholic liver in yellow fever means death.

The time allowed for the reading of this paper necessitates my leaving out much that I should like to discuss. I must ignore the interesting history of the disease and hasten on.

A few words or points on the after-treatment. The treatment during convalescence calls for constant watchfulness. It is here, that malarial symptoms crop up, in the cases of those who had been at Panama a few months. Dr. L. Girerd examined the blood of hundreds on arrival, and found it normal, in no case showing the malarial bacillus. After the first month he re-examined scores of them; the blood of all showed it, simply confirming the statements to be found in Dr. Tomes' work, "Panama in 1855," statement amplified in Dr. L. Girerd's work.

To return to the stage of convalescence, I have known a beefsteak to cause death on the tenth day. During convalescence such patients are simply ravenous. Well do I recall my own intense hunger. Slops are in order, fluid food, given at short intervals, not to overload the stomach. Its irritability lasts for weeks and weeks. Bathing, a thorough washing of the patient's body and hair daily in a weak carbolic bath, the thorough disinfection of the patient's effects and room.

The majority of cases were fatal on or before the fifth day, closing with the black vomit, suppression of the urine, etc. In such patients it was fever of a single "access," or paroxysm. Other cases passed through the "period of calm," and died in the third stage, or that of "secondary fever," from the sixth to the ninth day. Cases of typhoid character were rare. I saw but one, being that of my friend Dr. Arthur Gore, now in San Francisco, California.

The sequalæ: boils, pimples, parotid swellings, and intermittent fever, jaundice—I was of a rich canary color. It lasted a whole month. People were never curious about it or anxious to ask me questions—not any.

Now for a very brief reference to *post-mortem* appearances. My small experience under this heading simply confirms what an old and clear-headed American writer has stated, "Yellow fever has no pathology." I refer to Dr. Grenville Dowell, whose little *brochure* contains a mine of information, or what the great French Undertaker, M. DeLesseps, calls "an arsenal of facts."

The *post-mortem* findings are so variable in patients cut off by the same symptoms, that no reliance can be placed upon them. I deem it a blood disease, pure and simple, and, if my view is accepted, the absence of any marked pathological change, save in the blood itself, cannot cause surprise.

The liver: It presents a variety of conditions. I have found it fatty; again, fatty on section, showing an immense quantity of oil globules; again, perfectly normal in size and color. The chamois-colored liver is supposed to be the characteristic liver. I never saw but one, and that was the only one in nearly one hundred autopsies made at the Canal Hospitals, Panama, by Dr. S. Didier, a gentleman profoundly versed in yellow fever. He was born in one of its habitats, the island of Martinique, French West Indies.

The kidneys: Nothing constant. I met them large and small; again, perfectly normal to the eye.

The stomach: This organ presents signs of acute inflammation. Generally its coats were thickened; it contained more or less black vomit; I saw nearly a pint in one case; its inner surface showing innumerable pink points or *foci* of congestion, and small deposits of blood. Dr. Castellanos, a physician of the Charity Hospital, Panama, a Spaniard, told me that it was the only constant condition found by him, and he, while living in Cuba, had made nearly 150 autopsies.

The brain I have never examined. Dr. L. Didier found nothing worthy of remark in his large experience. Nothing.

The blood: I have always found it in a perfectly fluid condition. Remember the destruction of the blood and the great amount of albumen eliminated by the kidneys. Its specific gravity taken by me two hours after death, was nearly normal. To this fluid we must direct our whole attention. To repeat, I consider it a blood disease, pure and simple, and have held this view since 1884. Death in these cases is due to a true necremia. If this view, which I believe is peculiar to myself, be proven, we have an explanation of a majority of the symptoms of yellow fever, and as already stated, it explains the absence of any characteristic pathological changes, save in the blood.

The brain symptoms are due purely and simply to the destruction of our oxygen-carriers, the red corpuscles. The great Virchow attributes loss of consciousness to their failure to carry oxygen. By rendering the blood uninhabitable to the germs that prey upon and destroy the corpuscles, we triumph. Much remains to be explained about yellow fever. Many honest and patient toilers are at work on this great problem. I believe that with the discovery of the specific germ by Dr. Domingos Freire, of Rio de Janeiro, Brazil, by Dr. L. Girerd, at Panama, and its discovery by Dr. Carlos Findlay in Havana—to his and the work of his friend Dr. Delgado, of that city—add to this, our knowledge of the truly wonderful strides made by these gentlemen in their bacteriological studies and inoculations—to the above, by acidulating the blood, as I have done, where it has invaded the system—with such factors, the future seems full of hope to me. May it prove so. Having digressed, I must go back to the *post mortem* findings.

The bladder: Generally a few drachms of highly albuminous urine were found. Remember the suppression.

Black vomit has a peculiar odor, and is slightly acid to the taste. To clear up a vexed point in my mind, I collected some in one of my cases and tasted it. It required a little courage, but I was in earnest and working for results. I may state *inter alia* that it will never compete favorably with other beverages. The "vomit," on settling, deposits coffee-ground "particles," the fluid above being the color of weak black tea. Black vomit is not bilious vomit. I tasted it to clear up this very point. Black vomit as a symptom is of grave import.

It indicates blood changes—the beginning of the necremia. While at Panama I sent my friends specimens of my late patients. My rooms were miniature graveyards. Some "black vomit" sent to my old classmate, Dr. Wm. Osler, then Professor of Clinical Medicine in the University of Pennsylvania, with other materials furnished *pabulum* for a lecture on vomited matters. To-day, he is Professor of Practice of Medicine in the Johns Hopkins University, Baltimore, Md., and Physician-in-Chief to the magnificent hospital of the same name.

To recapitulate: Now that Drs. Freire, Girerd, Findlay and Delgado have found the same germ, Dr. Domingos Freire being the first investigator, and its discoverer, to him the honor and credit are due. He caused others to work. Now that this has been accomplished, I firmly believe a new era is at hand, and that soon, this constant reproach to our profession, and much vaunted modern civilization, the sway of yellow fever, is about to receive its *coup de grace*. Inoculations will protect man against this awful disease as vaccine does against small-pox. Dr. L. Girerd proved his good faith in such a vaccine, if the term is permissible, by making attenuated cultures of the microbes of specific yellow fever, and by inoculating himself and without carrying it to its full protective influence, he allowed himself to be bitten by mosquitos (Dr. Carlos Findlay's discovery) that had been feeding on a man in the yellow fever ward of the Canal Hospital, a case of specific yellow fever, the fifth day, the mosquitos were disturbed and allowed to bite him. The result was a mild yellow fever. I translated his report, and it was published in the *Canada Medical Record*, Montreal, in the fall of 1886, together with an editorial.

With inoculations to protect and prevent, and the purely acid treatment where the germs have invaded the system—with these, and a strictly scientific quarantine, *à la* Dr. Joseph Holt, our profession can save the lives of hundreds of thousands in the future, who but for such means would die like rotten sheep. The acid, I believe, is a germicide in these cases. I like the term and make no apology for using it. Intermittent fever, as we well know, thanks to the crucial work of Leveran, Girerd and Osler, has its *bacillus malariæ;* quinine is its germicide.

When these things are thoroughly understood and put in practice, travel in the tropics will be divested of its terrors. People ere visiting them for business or pleasure will be inoculated, and with quarantines, *à la* Dr. Joseph Holt, the commerce of nations will be almost free and untrammeled.

Here, gentlemen, I must say farewell. I have to thank you for your kind attention and patience. In the near future, I trust that you will recall these statements made in the presence of the Members of the State Medical Society of Arkansas, on this, the 28th of May, 1889.

Articles extensively quoted in preceding, from author's papers as under:

"Yellow Fever Considered in its Relation to the State of California." Ninth Biennial Report of the State Board of Health, 1886. Sacramento.

"Cuba in its Relation to the Southern United State; its Danger as a Disease-producing and Distributing Centre." Tenth Biennial Report of the State Board of Health, California, 1888. Sacramento.

"The Present Tendency to Epidemics." Tenth Biennial Report State Board of Health, California, 1888. Sacramento.

"The Isthmus of Panama Considered as a Disease-producing and Distributing Centre." Tenth Biennial Report of the State Board of Health, California, 1888. Sacramento.

"The Holt System of Maritime Sanitation, or an Ideal Quarantine." Tenth Biennial Report of the State Board of Health, 1888. Sacramento.

CASES OF INFANTILE HEMIPLEGIA.

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The following fourteen cases of infantile hemiplegia have come under my observation in this institution, all but one being under treatment at the same time, in a total population of 419 patients. They exemplify most of the symptoms usually found in such cases, and are, perhaps, of sufficient interest to be put on record, although I cannot lay claim to any original discoveries in connection with them. The histories furnished at the admission of the patients are, I regret to say, so imperfect as to be worthless for the purposes of this article.

Case 1.—Margaret B., aged 30. Said to have become paralyzed at the age of eight months. The left side of the face is smaller than the right, and less strongly innervated. Left arm shorter and smaller than right; good movement of elbow,